

THE WILSON BULLETIN

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EDITORIAL

BY RECENT ACTION of the W. O. C. Council the next annual meeting has been set definitely for December 28 and 29 (Friday and Saturday), at Pittsburgh, Pa., in conjunction with the A. A. A. S. meeting. The meeting for 1935 will be held during the Convocation Week, with the A. A. A. S., at St. Louis, Mo. The 1936 meeting has been tentatively set for early fall at Sioux City, Iowa. The 1937 meeting will probably go with the A. A. A. S. to Indianapolis, Ind., though no action has been taken on this date.

WE HAVE BEEN very much impressed by the recent discussion in the *Canadian Field-Naturalist* on the matter of making field identifications of subspecies of birds. The question is of sufficient importance and interest to justify a detailed review in this place. It is a matter of fundamental importance to all who undertake to prepare a list of birds based upon field identification—the so-called “sight records”. Is the student justified in listing subspecies on the basis of field identification?

The discussion began as the result of the publication (*Canadian Field-Naturalist*, March, 1933, page 56) of a Christmas Census (referred to in the discussion as the Comox Census) in which subspecies were enumerated. In the same periodical (for September, 1933, page 112) Mr. Hamilton M. Laing offers criticism which brings the question to an issue. Following this Dr. Harrison F. Lewis, as Chairman of the Bird Census Committee, makes a defense for editorial correction of the original manuscript. And on page 116 (*ibidem*) Mr. P. A. Taverner comments as the ornithological editor of the magazine. The discussion is continued in the December number (pp. 176-177) by Mr. W. E. Saunders and by Mr. Theed Pearse, author of the original Comox Census. We may now briefly summarize the contentions of the several authors.

The gist of Mr. Laing's criticism is that it is impossible to make the fine distinctions in the field necessary for identification of subspecies. Dr. Lewis republished the original Comox manuscript *verbatim et literatim*. By comparison of the original manuscript with the published list it is evident that the editor made certain vital changes. For example, “Chickadee” was changed to “Oregon Chickadee”; “Robin” was changed to “Northwestern Robin”; “Golden-crowned Kinglet” was changed to “Western Golden-crowned Kinglet”; “Purple Finch” was changed to “California Purple Finch”.

Thus, the author did not attempt to identify subspecies in the field, but was made to appear to do so by editorial prerogative. The reader may decide for himself which subspecies is more likely to be found in the given area, but the author did not make a decision on this point. It is many times a question how far it is proper for an editor to go in changing the author's meaning; but it is probably a safe rule for the editor to change to a weaker, rather than to a

stronger statement. The proper balancing of the rights of the author with editorial rules and style requires some care, possibly some skill. However, this is not the main point at issue. The main point is expressed by Mr. Taverner in the following words:

“The discussion brings prominently into view one of the most serious defects of the Fourth Edition of that Check-List [the A. O. U. Check-List]. It does not provide specific entities for just such uses as this . . . there is no way, without awkward circumlocution, of referring to many groups of subspecies or to designate forms whose subspecific status may be uncertain. . . . In formal use the scientific binomial is always available to the instructed but the general public who have little occasion to familiarize themselves with scientific technicalities are given no vernacular alternative but to make exact subspecific designation whether they are justified in doing so or not.”

Continuing the discussion Mr. Saunders asks, “Why, then, should we carry on the farce of naming the sub-species of birds seen in the field? . . . Sub-species are for the closet student, not for the field worker.”

Possibly these excerpts will give a sufficient idea of the trend of the discussion. We will not attempt to foretell what our own future editorial policy will be, except that we will try to be more careful. But we are disposed to recommend to prospective authors that faunal lists based upon field work should be reported in binomial terms rather than in trinomials. It is so perfectly evident that subspecific identification made in the field is pure guess work, that it really ought to be abandoned. We believe that writers usually assume that a bird belongs to a certain subspecies because it occurs within the usual range of that subspecies. This assumption is unscientific. As Mr. Taverner has said (WILSON BULLETIN, XL, December, 1928, page 263), “It gives a pleasing appearance of scientific acumen and accuracy that is lacking in fact. If we base our distributions on determinations in faunal lists and other records, and then make those determinations from such supposed distributions we work in a vicious circle that gets nowhere and confirms what error there is without a chance of correcting it.”

We may say that the WILSON BULLETIN, for the present, will not decline to publish such lists in trinomials if the author so prefers; but we strongly recommend and urge that binomials be used. Of course this presents certain difficulties. It will not be difficult, in most cases, to form the technical name of the species by simply dropping the third term in the trinomial name as given in the A. O. U. Check-List. But this Check-List as now written is utterly useless in providing specific vernacular names for our birds. It seems very strange now that the A. O. U. Committee should have been so short-sighted in failing to supply so obvious a need. The situation places upon each writer the responsibility of forming as best he may an appropriate vernacular name for each species.

The following extracts from a letter written to an author within the past year further explain our editorial position on the matter of publication:

“One can easily recognize a Bohemian Waxwing in the field, but one can not possibly say with scientific accuracy that it belongs to the subspecies *pallidiceps*: all that can be said is that there is a strong probability that it belongs to this race. And the reader can make this assumption as well as the author can. Hence, why not let the reader take the responsibility? And in cases where

there are several subspecies on the same continent the uncertainty becomes all the greater.

"However, until our editorial policy is shaped up more definitely, the author will have full sway with trinomials. . . . Please remember, that the proposition is that you can not recognize subspecies in the field in *any* case; that in every case listing subspecies in the field is guess work. This enables one to be consistent. The one difficulty is the lack of an English name for the species, and this is because the last edition of the Check-List pre-empted the old vernacular names and applied them to subspecific units." We hardly see how there can be an argument on this point. Because a given subspecies *ought* to be in the geographical range assigned to it, it is not valid scientific evidence that it is the one actually found there except where the specimen is taken and demonstrated. And the case permits of no generalization in fact beyond the fact that a given area is one where a given subspecies normally occurs. That every individual of the species found within the area belongs to the expected subspecies by virtue of its presence there is an unwarranted conclusion. If birds possessed less efficient locomotor organs the case might not be so clear. But even plants get out of their range by one means or another. How much more likely are animals to do so, and birds above all others! If birds were less motile, and if subspecies were more easily identifiable there would probably be less uncertainty in the assumption of identity on the basis of geographic incidence.

Since the last issue of the BULLETIN was distributed we have received several communications calling attention to the fact that often species can not be identified in the field. Our remarks on page 208 (December, 1933) were unfortunately phrased if they implied that species might always be recognized in the field. At once we grant the point that some species are not readily identifiable in the field, possibly not at all by many observers, and perhaps in some cases not at all by any observers. We were more intent on the proposition that subspecies are not identifiable in the field.

The difficulty has been forced upon us by a sort of orthogenetic bias of the taxonomic specialists who conceive the subspecies unit to be the *summum bonum*, and who, apparently, in their zeal to emphasize subspecies, have carelessly scuttled the species concept. The American Ornithologists' Union could perform a great service, if they would, by preparing a supplement giving vernacular specific names. This would tend to preserve uniformity, which under present conditions is likely to suffer.

THE NORTH DAKOTA list in this issue is published by the aid of a subsidy. This paper received the Sigma Xi certificate of award for undergraduate research, to which a reference is made, without names, in *Science*, February 2, 1934.

READERS of this magazine will be much pleased to know that Mrs. Nice has just published a very extensive paper on the natural history of the Song Sparrows in the *Journal für Ornithologie*. The first instalment appeared in the October number (Vol. LXXXI, No. 4, pp. 552-595); the second instalment has just appeared in the January number (Vol. LXXXII, No. 1, pp. 1-96). This paper presents a full account, in the German language, of Mrs. Nice's work to date on this species.